SCIENCE

VOL. LXXI — FRIDAY, FEBRUARY 28, 1930 — No. 1835

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SCIENCE: A Weekly Journal devoted to the Advancement of Science, edited by J. MCKEEN CATTELL and published every Friday by

THE SCIENCE PRESS

New York City: Grand Central Terminal
Lancaster, Pa. — Garrison, N. Y.

Annual Subscription, $6.00 — Single Copies, 15 Cts.

SCIENCE is the official organ of the American Association for the Advancement of Science. Information regarding membership in the Association may be secured from the office of the permanent secretary, in the Smithsonian Institution Building, Washington, D. C.

THE MOTIONS OF THE B STARS[1]

By Dr. J. S. PLASKETT, F.R.S.

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THE radial motions of the B type stars have been discussed by Frost and Adams, Campbell, Kapteyn, von Pahlen and Freundlich and by many others. Frost and Adams, from the radial velocities of twenty B type stars, were the first to direct attention to and Campbell, from much greater observational material, to confirm the results of these early observations, that the peculiar radial velocities, the velocities remaining after the component of the solar motion was removed or the actual velocities with respect to the stellar system, were much smaller for the B stars than any other spectral class, and further, that there was a large excess of velocities of recession, positive velocities, on the average nearly 5 km per sec., as if the system of B stars was expanding with respect to the sun.

[1] Address of the retiring vice-president for Section D—Astronomy, American Association for the Advancement of Science, Des Moines, Iowa, December 31, 1929.

This K term, as Campbell called it, which in his final determination had a value of +4.9 km per sec. and which was practically non-existent in other spectral types than B, caused considerable speculation. Attempts to explain it as due to a personal equation in the measurement of the broad-lined B type spectra, to errors in wave-length or to a red displacement of the effective center of absorption produced by some physical cause could not be made to account for more than a small part of the 5 km observed. The Einstein redward shift of the spectral lines of massive stars was later brought forward as a cause of this mysterious effect but on no probable assumption of mass and density could the displacement thus produced exceed a fraction of a kilometer.

A new departure in the explanation of this mysterious K term appeared a few months ago in the *Potsdam Publications* in a paper by von Pahlen and Freundlich who claim that it represents a real motion

of the B stars and is produced by an orbital motion of the stars around a very massive center, the center of the stellar system. On their hypothesis these orbits are very eccentric, e = 0.9 or so, and the sun and the cluster of B stars have just passed the apogalactic point, the point most distant from the center. As the orbital velocity will increase as the stars approach the center of the galaxy, the B stars preceding the sun will be moving faster and those following slower than the sun, thus giving an apparent expansion, a K term. They were thus able to explain not only the distribution of the then known residual velocities of the B stars but also other systematic motions of the stars in general, but this could be done only by rather special and somewhat artificial assumptions.

Objections may be urged to several of the hypotheses and methods used by von Pahlen and Freundlich, but this is scarcely necessary as the introduction of additional material, the radial velocities of about 450 fainter B stars obtained at the Victoria Observatory, so changes the nature of the observational data on which their investigation was based as to require a complete revision of the theory. As will be seen later, the motions of the B stars are well and simply explained by a general rotation of the stellar system around a massive and distant center, the orbits of the individual stars being nearly circular instead of highly eccentric as in von Pahlen and Freundlich's investigation.

The material for the present discussion of the motions of the B0 to B5 stars, to which it has been limited, as the classes B8 and B9 are generally taken as belonging to A type stars, has been gradually accumulating at Victoria since 1924 in the determination of the radial velocities of all B0 to B5 stars brighter than 7.5 visual magnitude and north of −11° declination, by Mr. J. A. Pearce and the writer. There were about 450 stars in this program, which with those previously determined gave the radial velocities of 815 B0 to B5 stars, nearly 500 more than were available to Campbell or von Pahlen and Freundlich. These 815 radial velocities or rather the 815 residual velocities, the velocities after removal of the component of the solar motion, the velocities with respect to the stellar system, were analyzed with particular reference to the theory of a rotation of the galactic system as introduced by Lindblad and further developed by Oort and the writer.

This theory assumes that the great galactic system is in practically circular rotation around a very distant and massive center supposed to coincide with the center of the globular cluster system at galactic longitude 325°. If, as seems likely from analogy with the spiral nebula, there is a condensation of the matter or stars towards the center, the law of force will approach that of the inverse square, the stars nearer the center will revolve more quickly than those farther out, somewhat similar to the solar system where the inner planets move much more quickly than the outer. In such a case, it is obvious there would be a relative motion of the stars nearer to or farther from the center with respect to the sun, or a rotation of the galaxy could be measured.

Oort analyzed the consequences of such a rotation and showed that the residual velocities were proportional to the distance from the sun and varied with the sine of twice the angle between the star and the direction to the center. Expressed mathematically, all positions being referred to galactic coordinates:

$\bar{\varrho} = K + \bar{r}A \sin 2(\bar{l} - l_o) \cos^2 \bar{b}$, where

ϱ = Mean residual velocity, the velocity with solar motion removed

$\bar{l}$ = Mean galactic longitude

$\bar{b}$ = Mean galactic latitude. All these barred terms refer to the average of a small group of stars, this grouping being necessary on account of the peculiar or random motions of individual stars.

K = Mean residual velocity with regard to sign

$\bar{r}$ = Average distance of the whole group of stars

A = Rotational term, about 1 km per sec. for stars about 60 parsecs, 200 light years away

l_o = Direction to the center of rotation, generally taken as galactic longitude 325°, the direction of the center of the system of globular clusters.

Using this relation and the method of least squares, Oort showed that the motions of the more distant stars were very similar to those that would be produced by such a rotation of the galaxy. This was further confirmed by an analysis of the motions of part of the B stars discussed in the present paper which was presented by the writer to the Royal Astronomical Society two years ago. The galactic rotation is still more strongly confirmed by the investigations of the radial motions of the 815 B0 to B5 stars used in this paper, and is specially marked in the most distant stars of the list where, as will be seen later, the K term disappears and the motions follow almost exactly the double wave swing produced by a galactic rotation. In a paper shortly to appear in *Monthly Notices*, the velocities of the most distant stars, and particularly the velocities of the intervening diffuse matter in the stellar system, as determined from the "interstellar" H and K lines in the spectra of these stars, show this rotational effect so distinctly and certainly as to leave no doubt of its objective reality.

The presence of this systematic effect in the radial motions of the B type stars makes it necessary, before we can speak intelligently about a residual velocity, a K term, to remove this double wave rotational term

from the radial velocities in addition to the usual single wave solar motion which was the only one known and allowed for by Campbell. It is only when all known systematic motions of the stars are removed from the radial velocities that the true residual velocity, the real K term, can be obtained, and that any explanation of this mysterious effect can be final. Consequently, in order to find the real K term for the small groups rather than individual stars in the solutions, as thereby some of the irregularities caused by the peculiar or random motions of the stars are avoided.

These six separate main groups were solved by the least squares, using the equation given above, with l_o taken as 325°, and yielded the following values of $\bar{r}A$ and K.

TABLE I
SOLUTIONS FOR $\bar{r}A$ AND K

Mag. Intervals	Types	No. Stars	Mean Mag.	No. Long. Div.	$\bar{r}A$	K
0 to 5.5	BO to B2	93	3.95	13	+ 2.1 ± 1.6	+5.8 ± 1.0
	B3, B5	297	4.65	18	− 0.4 ± 0.9	+4.8 ± 0.6
5.5 to 6.5	BO to B2	39	6.04	7	+16.3 ± 1.8	+0.2 ± 1.2
	B3, B5	157	6.07	10	+ 2.8 ± 1.2	+1.4 ± 0.8
6.5 to 7.5	BO to B2	53	7.15	9	+13.2 ± 2.0	−0.5 ± 1.4
	B3, B5	176	7.01	10	+ 3.6 ± 0.8	−0.5 ± 0.6

B type stars, the 815 observed radial velocities may be either analyzed simultaneously for the solar motion, the galactic rotation and the K term or preferably, as the stars are unsuitably distributed for the accurate determination of the solar motion, the component of the usual solar motion may first be removed and then the remaining 815 residual velocities analyzed for the galactic rotation and the K term.

It seems evident, however, before proceeding with the solution, as the rotational term is directly proportional to the distance from the sun and varies with the direction or longitude, that it will be necessary to divide the stars into groups at different distances and then further subdivide into smaller groups in galactic longitude. It has long been known that the B0 to B2 stars are considerably more luminous and hence more distant than the B3 and B5 stars, thus making two main distance divisions, containing 185 B0 to B2 and 630 B3 to B5 stars respectively, which should be separately considered. Each of these divisions was arranged in three groups according to apparent magnitude and hence, assuming small dispersion in absolute magnitude, according to distance. The first group contains all stars, 390 in number, brighter than 5.5 visual magnitude, and includes practically all those available to earlier investigators. The second contains 196 stars between 5.5 and 6.5 and the third 229 stars between 6.5 and 7.5 or fainter magnitudes. Each of these six groups was subdivided into a number of subgroups, in longitude, varying from 7 to 18, as shown in Tables I, II and III, and the mean longitude and mean residual velocity were obtained for each subdivision. It is desirable to use

The first five columns of Table I are self-explanatory and show graphically how the 815 stars are arranged into distance and longitude groups. The sixth column containing the values of the rotational term from the six main groups may preferably be first discussed. The $\bar{r}A$ for the two groups of brighter stars is small and practically indeterminate, owing partly to the brightness and hence the relative nearness of the stars but mainly, as will be seen later, to irregular systematic motions of certain groups of these stars. For the fainter stars, especially those of types B0 to B2, the rotational term is well defined, from 7 to 9 times the probable error, and corresponds, by the relation given previously of 1 km per sec. for stars 200 light years away, to average distances of the groups of about 3,000 light years, although for the B3, B5 stars fainter than 5.5 magnitude, the average distance is less than 1,000 light years. The smaller value of the rotational term for the faintest group of B0 to B2 stars may be ascribed partly to possible large random motions of single stars in the small groups but mainly to large dispersion in absolute magnitude so that apparent magnitudes do not necessarily represent relative distances.

That all these fainter stars show distinctly and certainly the presence of the rotational effect is at once evident when the mean observed residual velocities of the various longitude groups are compared, as in Table II, with the velocities computed from those values of $\bar{r}A$ and K obtained in the solutions and given in Table I, and below the columns in Table II. It is scarcely conceivable, especially when the possible disturbing effects of large peculiar or

TABLE II

COMPARISON OF OBSERVED AND COMPUTED VELOCITIES
B0 to B2 Stars

5.5 to 6.5 Mag.				6.5 to 7.5 Mag.			
No.	$\bar{l}$	$\bar{\rho}$(Obs.)	$\bar{\rho}'$(Comp.)	No.	$\bar{l}$	$\bar{\rho}$(Obs.)	$\bar{\rho}'$(Comp.)
1	338°	+ 7.3	+ 7.1	1	359°	+36.3	+11.7
6	32	+15.2	+11.7	6	40	+11.7	+ 6.2
5	48	+ 9.1	+ 3.9	6	50	+ 2.1	+ 1.9
7	77	-12.8	-10.0	9	69	- 5.6	- 6.6
3	134	+ 1.2	- 2.5	4	91	-19.8	-12.8
6	160	+ 6.6	+ 8.2	8	109	-12.3	-12.8
11	180	+12.6	+15.2	6	148	+ 5.5	+ 0.9
				6	168	+ 2.6	+ 8.7
				7	179	+ 6.3	+11.4
	$\bar{r}A$ = +16.3; K = +0.2				$\bar{r}A$ = +13.2; K = -0.5		
	B3, B5 Stars						
15	359	+ 5.4	+ 4.0	7	55	+ 5.9	+ 3.0
24	29	+ 0.9	+ 3.6	27	22	+ 3.5	+ 2.8
25	56	- 1.8	+ 1.4	20	37	+ 2.4	+ 1.6
15	76	- 3.5	- 0.4	18	56	+ 1.9	- 0.6
16	120	+ 1.0	- 0.7	26	76	- 4.0	- 3.0
23	164	+ 6.2	+ 3.2	19	93	- 5.7	- 4.1
21	177	+ 3.1	+ 3.9	16	126	+ 0.6	- 2.8
11	208	+ 3.8	+ 3.7	13	148	- 0.8	- 0.2
5	245	+11.9	+ 0.5	20	167	- 2.2	+ 2.0
2	307	+ 5.1	- 0.2	10	182	+ 2.8	+ 3.0
	$\bar{r}A$ = +2.8; K = +1.4				$\bar{r}A$ = +3.6; K = -0.5		

random velocities on the mean residual velocities of groups with few stars is considered, that such a close agreement between observed and computed velocities can be accidental. If the double wave swing of the observed velocities from positive to negative and back again to positive is not produced by a galactic rotation it must be due to some cause which gives a systematic trend of the velocities almost exactly similar to a galactic rotation.

It is evident, therefore, that these rotational effects should be removed from the observed radial velocities in addition to the solar motion. Obviously this has been done in the solutions, and the remaining residual velocity, the true K term, is shown in the last column of Table I. Its value for the 93 B0 to B2 stars brighter than 5.5 magnitude is +5.8 km and for the corresponding 297 B3, B5 stars is +4.8 km per sec. It is therefore little different from Campbell's value of +4.9 km per sec., obtained from practically the same stars. It has, however, completely vanished, at least well within the probable errors of the determinations, for the stars fainter than 5.5 magnitude. That is to say, while the application of the galactic rotation to the residual velocities of the B stars has completely eliminated the mysterious K term from the 425 B stars fainter than 5.5 magnitude, this K term of about 5 km per sec. still persists for the 390 B stars brighter than 5.5 magnitude.

It is obvious that all the early explanations of this K term as caused by errors of measurement, errors of wave-length, displacements due to physical causes or to the Einstein shift, or the later explanation provided by the hypothesis of von Pahlen and Freundlich must fall to the ground, as it is inconceivable that these causes should not operate in exactly the same way and to the same degree for faint as for bright stars. To imagine any physical property of this uniform class of B stars which depends only on the apparent brightness or, in other words, on the distance from the sun, an insignificant member of the stellar system only about one thousandth as luminous as the B stars, is to carry anthropocentric prejudices to an absurd degree.

The explanation of the K term, therefore, must be sought in some peculiar or systematic motion of these brighter B stars, or of certain groups of these stars. It is well known that the B stars show marked clustering into groups in position and it is not unreason-

able to suppose there might also be some systematic trend in the motions of these same groups. In the subdivision of the stars brighter than 5.5 magnitude into longitude groups, advantage was taken of natural separations so that these subgroups may be assumed to correspond approximately to the natural clustering of the B stars. Some information in regard to such group motions may therefore be obtained by a comparison of the observed residual velocity $\bar{\varrho}$, with the computed, ϱ', of the two groups of stars brighter than 5.5 magnitude, which is given in Table III. This comparison shows first of all no indication of the agreement so conspicuous in the fainter stars. This is partly due to the relative nearness of the stars to the sun thus making the rotational term small and indeterminate, but mainly to group or cluster motions of recession of some of the southern stars.

The great preponderance of relatively large positive observed velocities beyond longitude 200°, in the southern sky, provides an obvious reason not only for the low values of $\bar{r}A$ but for the high values of K. It can be safely assumed from the agreement between observed and computed velocities for the fainter stars in Table II that there are positive rotational terms for these two groups of brighter stars also, which are masked by the distribution of the velocities. It may be assumed from the relative brightness and distance as compared with the fainter stars, that $\bar{r}A$ for the bright B0 to B2 stars should be about +6 km and for the B3, B5 about +3 km. Further assuming the K term as half the computed values or +3 and +2 km respectively and again computing velocities we get the columns ϱ''. Although without objective reality, they are still useful in indicating a possible representation of the velocities and showing that the groups mainly responsible for the high K term are those between 240° and 290° longitude.

Thus the low and uncertain values of the rotational term and the high values of the K term for these brighter stars are probably due to the high systematic recessional velocities, probably group or cluster motions, of limited groups of the southern stars, mainly between galactic longitudes 240° and 290°. This is made more probable by the relative brightness and nearness of these stars, the B0 to B2 stars in the above longitude limits being more than a magnitude brighter than the average, with a similar though smaller difference for the B3, B5 stars.

There can be no reasonable doubt, therefore, that the mysterious K term, this positive residual velocity of 5 km per second found by Campbell for the B0 to B5 stars brighter than 5.5 visual magnitude, is due mainly to the high velocity of recession of limited groups of the brightest B stars in the sky in galactic longitudes 240° to 290° in the Vela-Lupus region.

TABLE III

COMPARISON OF OBSERVED AND COMPUTED VELOCITIES

Stars brighter than 5.5 Mag.

B0 to B2 Stars					B3, B5 Stars				
No.	$\bar{l}$	$\bar{\varrho}$	ϱ'	ϱ''	No.	$\bar{l}$	$\bar{\varrho}$	ϱ'	ϱ''
3	33°	+18.3	+7.0	+6.3	9	355°	+ 1.6	+4.5	+4.3
5	64	+ 3.3	+5.2	+1.6	7	12	0.0	+4.5	+4.4
7	87	+ 1.8	+4.1	−1.9	19	32	+ 2.4	+4.6	+3.8
6	133	− 0.3	+5.0	+0.7	15	49	+ 2.0	+4.8	+2.5
7	162	+ 2.5	+6.9	+6.2	11	68	− 1.6	+5.0	+0.8
14	177	+ 6.7	+7.6	+8.2	11	93	− 0.3	+5.2	−0.6
8	203	+11.2	+7.0	+7.8	16	117	− 1.1	+5.2	−0.4
10	230	+ 9.3	+6.2	+4.1	15	136	+ 0.7	+4.9	+1.2
5	268	+12.3	+3.9	−2.3	18	156	+ 7.2	+4.7	+2.9
10	291	+ 7.6	+4.0	−2.0	20	175	+ 4.1	+4.5	+4.4
4	309	− 9.0	+4.7	−0.2	12	195	+ 8.5	+4.5	+4.6
11	320	+ 5.6	+5.4	+3.0	22	212	+ 7.6	+4.6	+4.0
13	344	+ 1.3	+7.0	+6.3	21	230	+ 5.3	+4.5	+2.5
					24	249	+ 6.3	+5.0	+0.7
					25	271	+10.2	+5.2	−0.8
					24	293	+ 7.8	+5.2	−0.5
					19	315	+ 7.8	+5.0	+1.1
					9	329	+ 5.0	+4.8	+2.3
		$\bar{r}A = +2.1$; K = +5.8					$\bar{r}A = -0.4$; K = +4.8		

That this high excess positive velocity appears only in these particular southern groups and is absent in the northern groups of the brightest and nearest stars, and is also completely absent in all B stars fainter than 5.5 visual magnitude, shows conclusively that the mysterious K term has no particular relation to the B type stars, is not due to any physical displacement of the spectral lines or to erroneous wavelengths and does not require the elaborate explanation of von Pahlen and Freundlich, but is probably caused entirely by an unsymmetrical distribution of the residual velocities, by a preponderance of high recessional group velocities in the Vela-Lupus region.

The average peculiar motion of the B stars, the mean residual velocity without regard to sign, after the removal of the effects due to solar motion and galactic rotation, is found to be somewhat greater than that obtained by Campbell, and varies between 9 km per second for the bright B stars to about 12 km for those fainter than the seventh magnitude. It is still considerably less than the average peculiar velocity of stars of other spectral types, and the stars of the spectral class B0–B5 may be considered as relatively slow-moving stars, as having smaller peculiar or random velocities than any other spectral class.

In conclusion, this investigation of the motions of the B0 to B5 stars has shown that their general residual velocities, the velocities with respect to the stellar system, agree very closely with those that would be produced by a rotation of the galactic system around a very distant and massive center, thus increasing the probability of such a rotation. It has further shown that the excess positive residual of about 5 km per second found by Campbell and others has entirely disappeared, when the effects of the galactic rotation have been removed, for all the B0 to B5 stars fainter than 5.5 magnitude. Although the K term still remains for the stars brighter than 5.5 visual magnitude an analysis of the residual velocities shows conclusively that this final residual of 5 kms is mainly due to the high positive group or cluster motions of the brightest and nearest B type stars in the sky in the Vela-Lupus region and is no general characteristic of the B type stars as a class. Finally, the investigation has shown that the average residual random or peculiar motions of the B0 to B5 stars varies from about 9 km for the brighter to 12 km per sec. for the fainter stars. While considerably higher than values previously deduced, the peculiar velocities of the Bs are still less than those of any other spectral class.

It gives me pleasure to acknowledge the effective collaboration of Mr. J. A. Pearce with the writer in the observation and measurement of the spectrograms and in the calculation of the results.

ORGANIC EVOLUTION[1]

ITS PROBLEMS AND PERPLEXITIES

By Dr. ALEŠ HRDLIČKA

AFTER cosmogony, the greatest phenomenon in nature is organic evolution.

There is no need any more of attempting to substantiate this great process. Natural history teems with its evidence. And this evidence is so convincing that some scores of thousands of men of science, who represent the most critical minds of the present human society, are without exception deeply convinced of its reality.

This does not mean, however, that this most comprehensive and complex subject is as yet well known in all its details, or understood. What the workers in natural sciences are deeply conscious of is the substantiality of this all-pervading phenomenon. They further feel that gradually they are learning its accomplishments, and some of its principles. But they are also deeply aware that they are still far from knowing all its details or processes and especially far from comprehending its essential causes and significance.

[1] Address of the retiring president of the Washington Academy of Sciences, Washington, January 14, 1930.

Let us survey briefly some at least of the dark areas of the field and some of its perplexities.

We may as well begin by trying to define evolution —and we are at once in serious difficulties.

There have been many attempts at such a definition; they have all failed. How even now they fail may be appreciated from two of the latest attempts.

For H. S. Jennings (1928), "The doctrine of organic evolution is the doctrine that animals and plants are slowly transforming, producing new kinds"; while for David Starr Jordan (1928), evolution is merely "the universal process of orderly change." How inadequate are these expressions, how partial; yet it seems impossible for the present to do much better.

The fact is that the totality of the great subject is not yet graspable. Every worker sees it mainly from his angle, while the phenomenon as a whole is as comprehensive as that of life itself—the two may even be synonymous, or one the discharge of the other.

An approach towards the understanding of organic evolution lies necessarily in ever-progressing, intensive research and study; research and study of its

manifestations through time, of its processes to-day; and on the basis of these of its laws and principles.

As matters stand now, what seem to be the more obvious of such laws or principles?

The first such apparent law seems to tell us that every organic unit has and can be derived only from a closely related previous unit. This is the principle of related parentage. It is a much more important principle than may at first be appreciated. It implies that there are no new without previous closely related forms. There are mutations but no free saltations, no unconnected origins. This law, if it proves fully to be such, implies the principle of organic continuity, of the continuity of the organic with the suborganic world, and of the continuity of this with the inorganic. And it implies the great principle of heredity or transmission of the bulk of parental characters.

The next fundamentality is that every organic unit differs congenitally more or less from every other unit, and that such differences tend to be transmitted to the progeny. These are the important laws or principles of variation, and of inheritance of new characters.

The inborn variations of characters—physical, physiological or mental—differ in their value to their bearer. Under given environmental or individual conditions some may be favorable, some effectless and some unfavorable to the organism. In the long run, the units or groups with favorable variations will tend to prevail—which is Darwin's famous, and now as true as ever, principle or law of natural selection.

Every organic unit is plastic or impressionable, and reactive or accommodable. Use and disuse of parts or the whole, and environmental agencies, cause alterations that, if the causes persist long enough, will pass from temporary to habitual, and eventually to permanent changes that will be inheritable. The principles here involved are adaptation and the fixing of adaptations. The latter is the main and undoubtedly correct principle of Lamarckism and neo-Lamarckism. It is not opposed to but cooperative with natural selection and other factors of evolution. It should more properly be known, however, as the fixing of adaptations, rather than by the old term of "inheritance of acquired characters."

And a progression or differentiation of a filial group, with the additional aid of the important factors of sex, hybridization, isolation, etc., until every member of it can be distinguished from those of the parental group, and until a return to the parental group becomes impossible (A. H.), constitutes speciation or particular evolution.

So far, all is relatively easy sailing, and in well-traveled waters. The processes involved are, in the main, fairly understandable. There is thus some grasp of what may be designated especial, individualized or particular evolution.

But when we contemplate the organic world as a whole and from its beginnings, we are confronted with a far greater phenomenon, which is a general progressive evolution.

Here is something immeasurably more difficult to comprehend. It is no less than the sequential formation, the systematic building up, of the entire organic kingdom. And its causes and meaning are elusive.

What is that something that has led to the development of organic forms from the inorganic, and that within the organic realm has led unceasingly to progress in diversity, complexity, sensibility, effectiveness, until there was reached the culmination in a genus of creatures that are self-conscious, rationally directive, relentlessly striving for more knowledge, power, for co-creation even—the creatures we know as the humans?

This general progressive evolution may really be likened to a double-stemmed vast tree, rooted in the suborganic matter and the inorganic earth. One of these stems represents the vegetal, the other, statelier, the animal kingdom. As this latter stem, which concerns us more especially, grew upward, innumerable branches of it withered and were shed off, but the main stem never ceased rising and branching anew, until it reached the present highly differentiated crown and its human summit.

Here is before us an all-pervading something in living nature to which to apply the terms of accidental or incidental would be utterly trivial and absurd. Yet if not such then what? Then it must have been inevitable, and somehow predetermined in the organization of things. General organic evolution can only be, it seems, nature's function, resultant from nature's own potentialities, organization and evolution.

But we know very little as yet about these basic potentialities, organization and evolution.

General organic evolution, we feel strongly, is as natural a process as is the particular evolution of individual varieties and species. There is no indication within the process of anything except the natural. But "nature" (which may perhaps be crudely and somewhat metaphorically defined as the earth with all it stands for fertilized by the sun and the rest of the universe), in the light of organic evolution, assumes far greater riches and importance than it has hitherto been credited with.

We may now proceed to some of the more detailed problems and perplexities of the subject.

We may as well begin with the term "evolution." Is this a correct or the best term for the great process under consideration?

(1) The word "evolution," it is well known, means merely unraveling, unfolding. This would imply that all the potentialities, all the eventual results, of organic evolution lay already fixed in the most ancestral

units of the living kingdoms, in the earliest cells, and even in their predecessors.

General organic evolution under such conditions would be merely general ontogeny. If these concepts are followed far enough they lead to the absurd. The incongruity of the term "evolution" has been felt by many workers. There are those who prefer the term "transformism," which falls short. Morgan has coined "emergent evolution," which does not improve matters. Osborn suggests "creative evolution," which obscures rather than clears. There is but one term that would appear adequate to the speaker. In harmony with "cosmogony," general organic evolution, the greatest of organic phenomena, deserves the term of "biogeny."

(2) Did organic evolution begin with a cell? We used to believe it. Many who perhaps have not had the chance to go more intensively into the subject believe it still. But those who know the cell can not believe that the cell is the beginning of organic life. The cell is already a great accomplishment in organic evolution. It is a little cosmos of its own. It is extremely complex and full of highly differentiated activities. It is a carrier of all sorts of ancestral things. It would seem that a long road in biogeny had already been covered when there was reached the first full-fledged cell.

To-day it is known, moreover, that there are whole classes of ultra-microscopic existences which seem to behave like living beings and produce definite lines of results. There are a number of them already known, as shown recently by Flexner, in pathology, and there are probably many others in nature, non-pathological and perhaps, as in the case with many micro-organisms, even helpful. Shall these ultramicroscopic somethings be called cells? They seem too minute to deserve such appellation, too minute to possess the regular cell characters. If they are organic units, they are units of a lower order than that of the cells proper. They disclose what may be designated as a suborganic kingdom. These suborganic existences must be derived from something still simpler which, it would seem, could only be peculiar aggregates of organic molecules. And such aggregates of molecules would constitute the last link between the organic and the inorganic world. Much of this is still hypothetical, but progressing studies on the colloids, ferments, enzymes, viruses and other substances are yielding many suggestions.

(3) Has origin of organic life taken place but once, or has the earth indefinite capacities in this direction?

Are there possibilities of the arising of new classes of organisms and perhaps of new lines of evolution? It would be wrong to deny either of these propositions.

It is impossible to deny a continuous potency of nature to produce from the inorganic the suborganic, and from this the organic. But it is impossible as yet to show this, to prove it.

Sometimes we are confronted with the statement that there can not be any new "creation" for nothing of that sort has ever been witnessed. To this may be replied that a new suborganic or inorganic object would have, under present conditions, practically no chance of surviving. The world throughout is filled with forms that are eager for food. What chance of survival under such conditions would have a new limited generation of beings? That there is a possibility of such a development can not be denied. There may even be felt some apprehension. There might arise new scourges. Perhaps some of the scourges of the past could be explained thus. But even if this happened at this moment, a recognition of the new form as such would be exceedingly difficult.

It is quite probable that the biogenic powers of nature are still potent enough to produce new primordial forms that, if conditions were favorable, could in time develop into recognizable new phyla, but they could prevail only if sufficiently numerous and destructive to overcome already existing interfering forms.

As to whether any new major lines of organic beings may still develop, it seems impossible either to deny or assert. These processes are very gradual and observations of them are very difficult; and belief or disbelief alone count but little.

(4) Does evolution proceed in all organisms, and does it proceed with anything like a definite rate as to time?

All organisms are known to be impressionable. There is no organism that can not be affected by new stimuli or by changes of the old influence that acted upon it. So long as these conditions exist, so long will there be a capacity of further development, a capacity of evolution. It all depends upon the stimuli as to whether an organism will or will not evolve any further.

As to the rate at which organisms evolve, that, it is definitely known, differs greatly. There are organisms such as the medusae, ants, many other insects, some mollusks and still others that are known to have changed but little since the Cambrian, the Devonian or the earlier Tertiary times. On the other hand there are forms, of which man is one of the best examples, that have changed rapidly even since the last ice invasion. The rate of evolution therefore differs widely. There seems some possibility of an explanation of these differences. When an organism,

even man himself, gets fully or nearly fully adapted to its environment, then, until something new happens, some new factors come into play, that organism will simply keep on, like a light that keeps burning evenly. But turn on a greater supply of oxygen or furnish some other suitable stimulus, and the light will flare up with renewed life and activity. The rate of evolution in any given organic line is commensurate with the influences that act upon such line.

(5) Does evolution ever reverse itself?

Here is a problem which does not seem to be difficult to answer. There is no case known in which a fully developed species has reverted to an ancestral species. But single parts, and even whole variants, can and do revert. And a progress in efficiency may be accompanied by simplification of structure. Something of this nature seems, for example, to have happened in the diatoms.

The differentiation in given phyla or organisms is led by what may be termed the chief object of such organisms. The structural parts accommodate themselves to this chief requirement. The accommodation may be partly by growth, partly by simplification. And simplification is effected by degeneration of useless or interfering parts and their eventual elimination. Man is full of examples of all this. The living system does not favor anything that has ceased to be useful, whether it is an indolent human brain or an effete appendix; it weakens, reduces and eventually eliminates in one way or another.

(6) What are the relative rôles of mutation, or sudden jumps in evolution, and of gradual changes?

When de Vries discovered mutations in his Oenotherae, it seemed as if a new principle had been discovered in organic developments. To-day, when looked at more soberly, it is perceived that mutations and the more gradual changes are merely differing steps, differing rates, of the same process, except where mutations may be accidental, as they are time and again due to poisons, rays and mechanical causes.

Mutations, as seen to-day, are not as desirable as more gradual changes. They demand much greater accommodation of the organism to them, which may not always be "healthy" or even possible. A mutation may be of such a nature as to demand a substantial change of the organism. Accommodations to lesser changes are always easier. But mutations are not changes of a distinct order.

(7) In observing general organic evolution, do we perceive in the species, genera, families, orders, kingdoms anything like life cycles?

The life cycle, in its essentials, is a universal attribute of polycellular, if not all, organisms. It is something very fundamental and general. Is it limited to individuals—or does it extend also to the species and higher groups?

Some observers are inclined to see such group life cycles (*e.g.*, in mollusks), and to explain it as in the individual by a progressively diminishing capacity of oxidation.

It is impossible to say as yet anything definite on this point. There are slowly growing indications to the effect that there are in nature factors that act simultaneously on whole groups, whole varieties, whole species, and that such groups behave in unison, as sorts of superindividuals. But all this, as so much of what preceded, is still full of uncertainties and can be decided only by future investigations. We sense that there is something in this direction but can not yet be sure.

(8) The last problem I am able to approach in the time available is one that is essentially human. How far can man hope eventually to control biogeny—biogeny of plants and animals, biogeny of his own genus?

The promise is warm, if not yet glowing. We are gradually learning the ways of nature, and how to use its powers. There is a steady advance also in understanding as to what is and what is not desirable. Nature's chemical and physical laboratories, as well as our own, are ever more effectively at our disposal. Genetics and experimental science are already trying their hands in the biogenic fields, with as yet not large but growing results. When man shall have reached so far in knowledge as clearly to distinguish the right ways, the means to proceed along these will soon follow. With the advance in the scientific knowledge of nature there can be set no limit to man's possibilities. He may confidently be expected to become an enlightened coworker with nature. But this will not be greatly achieved in our nor yet the next generation.

OBITUARY

MEMORIAL TO PROFESSOR EDWARD S. MORSE[1]

FROM a Japanese friend I have notice of an event which may be of interest to the readers of SCIENCE who knew the late Professor Edward S. Morse. The following is in part adapted from the account which appeared in the *Japan Advertiser* early in November.

On the afternoon of Sunday, the third of last November, some two hundred scientists and scholars assembled at the shell-mound near the station at Omori for the ceremony of unveiling a monument in

[1] We learn with regret that the author of this contribution died suddenly on January 28.

honor of Morse, and in commemoration of the fact that the mound was the cradle of archeological and anthropological studies in Japan.

It will be remembered that Morse went to Japan for the first time in 1877. As he passed Omori on his first journey to Tokyo from Yokohama where he had landed, he recognized a shell-mound such as he had often investigated along the coast of Maine and elsewhere. He found that the significance of such heaps of shells was quite unknown to the Japanese, and that nothing of the kind in Japan had been noted by any foreign observer. With characteristic enthusiasm, he began investigating the Omori mound, and speedily identified it as a prehistoric kitchen midden. This investigation was the first of its kind in Japan; Morse's publication of it was the first of a series of archeological studies issued from the Imperial University, and the collection of implements and pottery from the Omori mound, arranged by Morse, was the beginning of the archeological collections in the Imperial University and in the Imperial Museum in Tokyo.

The monument which commemorates all this is in the form of a great stone slab, eight feet by five, upright upon one of its long sides, and inscribed "Omori Shell-Mound" in Chinese characters large enough to be read from the passing trains. In the middle of the upper long side is a granite jar shaped like the typical vessels found in the mound. It stands on a plinth above an inscription in honor of Morse and his contribution to science in Japan.

The memorial inscription was composed by Dr. Chiyomatsu Ishikawa, professor emeritus at Tokyo Imperial University, who was Morse's interpreter in the early days and one of his students. Associated with him in planning for the monument were two other students under Morse—Professor Chujiro Sasaki, of the Tokyo Imperial University, and Professor Tomotaro Iwakawa, of the Tokyo Higher Normal School—and a number of his friends of later years. The cost of the monument was borne by Mr. Hikoichi Motoyama, and it was designed by Prince Oyama, Dr. Arisaka and Mr. Sugiyama.

It may also interest the readers of SCIENCE who esteemed Morse to know that at the Museum of Fine Arts in Boston, where for the last thirty-three years of his life he was keeper of the Morse Collection of Japanese Pottery, the Edward S. Morse Memorial Fund was established in 1927–28 through the generosity of a large number of his friends. The income of that fund is used to augment the collections of pottery in the custody of the department of which Morse was a member, and the purchases made from it are distinguished as belonging to the Edward S. Morse Memorial Collection.

F. S. KERSHAW,
Keeper of Chinese Ceramics

RECENT DEATHS

DR. WILLIAM H. NICHOLS, chairman of the board of the Allied Chemical and Dye Corporation, past president of the American Chemical Society, the Society of Chemical Industry and the Eighth International Congress of Applied Chemistry, known for his work on the metallurgy of copper and in industrial chemistry, died in Honolulu on February 1. Dr. Nichols was seventy-eight years of age.

DR. JOHN HOWARD APPLETON, professor emeritus of chemistry at Brown University, died on February 18 at the age of eighty-six years. Dr. Appleton retired in 1914.

DR. WILLIAM OTIS BEAL, for seventeen years a member of the faculty of the University of Minnesota, and recently chairman of the department of astronomy, has died at the age of fifty-six years.

DR. GUY L. NOYES, dean of the school of medicine of the University of Missouri, died on February 4. He was fifty-seven years old.

JOHN N. COBB, dean of the College of Fisheries at the University of Washington, Seattle, died on January 13 at the age of sixty-two years.

MAX LATSHAW, Ph.D. (Johns Hopkins), died suddenly in Berkeley, California, on January 23, aged thirty-seven years. At the time of his death he was a research chemist on the staff of the Shell Development Company.

DR. ROBERT FULFORD RUTTAN, emeritus dean of the faculty of graduate studies and research at McGill University, died on February 19. He was seventy-four years old and had retired last year, after having been associated with the university since 1886. He formerly was director of the Canadian Advisory Council for Research.

DR. JAMES MATTHEWS DUNCAN SCOTT, professor of physiology in the University of Saskatchewan, died at Saskatoon on January 28.

EDWIN TULLEY NEWTON, F.R.S., until his retirement in 1905 paleontologist to the British Geological Survey and to the Museum of Practical Geology, London, died on January 28, within four months of completing his ninetieth year.

THE death is also announced of Dr. George Goudie Chisholm, Edinburgh, known for his work on geography, at the age of eighty years.

SCIENTIFIC EVENTS

THE UNIVERSITY OF CALIFORNIA ECLIPSE EXPEDITION

IN answer to a request by the University of California that the army air service cooperate with Lick Observatory in observations of the total eclipse of the sun, which will be visible in California on April 28, President W. W. Campbell has received word from the United States army headquarters in San Francisco that the requisite type of plane is available, and that such cooperation will be gladly offered.

Tentative plans call for the services of one plane at an altitude of not less than 10,000 feet, over Napa Valley, between Napa and St. Helena, and perhaps of another plane equipped to take moving pictures of the eclipse shadow and of the eclipse itself.

In explanation of the cooperative plan, President Campbell says:

> The purpose of the airplane plan is to carry the observer well above the highest clouds which might be in the way of eclipse observers located on the earth's surface. The total phase of the solar eclipse will last not more than one or two seconds. The shadow of the moon on the earth's surface will be not more than one half or five eighths of a mile in width, and for an observer to locate his observing station where the shadow of the moon in its rapid northeastern travel will pass directly over him and give him the second or two of totality, will be difficult, and it is quite likely that the slight uncertainties in our knowledge of the precise position of the moon will leave many intending observers a little too far north or a little too far south to have the shadow pass over them.
>
> The sky will not be very dark at the instant of totality because the moon will succeed in just a little more than covering the sun's image. I should guess that the observers who are fortunate enough to find themselves for a second or two in the shadow of the moon could read fine newspaper print about as easily in the open air as they would a minute or two before sunset on a clear day. This estimate, however, is very uncertain, but there will be no difficulty whatever in reading ordinary newspaper print at the time and place of totality.

The university is now preparing an observing station to photograph the eclipse from the earth's surface near Camptonville, Yuba County. This expedition, which will be under the charge of Dr. J. H. Moore and Dr. D. H. Menzel, is financed by William H. Crocker, of San Francisco, chairman of the regents of the university.

The plane to be used for high altitude observations will be of the open cock-pit type, of such construction that the upper wing will not interfere with the view of the observer or of the pilot. For a few moments before and after the eclipse, the plane will fly in the direction in which the shadow moves, northeastward, in order to increase the length of time that the total eclipse may be viewed, by a little period.

It is suggested that there might be some interest in the taking of moving pictures of the eclipse, as also of the shadow on the ground, providing that it has edges sharp enough to be defined. In spite of the fact that maps have been prepared, showing the calculated path of the eclipse as accurately as possible, President Campbell points out that there is a possibility, though hardly a probability, that the shadow may be missed in spite of all precautions.

BUILDINGS OF THE YALE SCHOOL OF MEDICINE

A GIFT to Yale University of $400,000 from the General Education Board for the completion of the Clinic Building on the grounds of the New Haven Hospital, which is affiliated with the Yale School of Medicine, was announced by Dr. M. C. Winternitz, dean of the Yale School of Medicine, in an address to the alumni on February 22. This sum supplements the gift of $2,000,000 made by the same board a year ago for construction purposes and makes possible the completion of both the Medical and Pediatrics Laboratory and the Clinic Building.

The Medical and Pediatrics Laboratory is now under construction. It will be connected on every floor with the new Raleigh Fitkin Memorial Pavilion, to be opened on March 1, and with the isolation pavilion. Excavation for the Clinic Building has been completed and the foundation is going in. The structure will be six stories high and will contain administrative offices, kitchens, dining rooms, storage, linen rooms, history rooms and pharmacy for the entire group, as well as examining and treatment rooms for out patients. The Medical and Pediatrics Laboratory will contain offices, laboratories and teaching facilities for those men on the staff concerned with non-operative therapies. There are now lacking only four units for the complete modernization of the New Haven Hospital group. These are the ward pavilions for surgery, diseases of women and contagious diseases, and the pavilion for semi-private beds.

The need for the further development of the site of the Sterling Hall of Medicine was also presented by Dr. Winternitz. It is proposed to extend the wing of the Sterling Hall of Medicine to Oak Street and to provide in this extension quarters for the psychobiology unit under Dr. Robert M. Yerkes, as well as increased facilities for the department of physiology, especially for the work in neurophysiology to be conducted by Professor John F. Fulton and Professor J. G. Dusser de Barenne, who have recently been ap-

pointed to the staff. It is also proposed to include here gymnasium, squash courts, club rooms and locker rooms for the faculty and student body connected with the Institute of Human Relations and the medical group. Dr. Winternitz stated that a further need which is being considered is that of a dormitory to be erected on property already owned by the university for the use of students connected with the Human Welfare Group.

In speaking of developments since the Institute of Human Relations was established a year ago, Dr. Winternitz stated that great progress had been made in the selection of personnel for strengthening already well-organized units affiliated with the institute. The institute, he said, will serve as a dynamo and assembling plant for those university organizations concerned from the view-point of research, teaching or treatment with problems of human well-being. The effectiveness of the institute therefore depends upon the strength of the associated units and the program during the past year has been designed for the development of these associated interests.

Construction of the Institute Building, for which $2,000,000 was given by the Rockefeller Foundation and the General Education Board, is now well under way. It is expected that the building will be ready for use at the opening of the next academic year. It will house the department of psychiatry and mental hygiene, the graduate divisions of psychology and of the social sciences, and the child development work under Dr. Arnold Gesell.

TRIBUTE TO THE WORK OF MR. C. DAVIES SHERBORN

THE following communication has been sent to the directors of the British Museum of Natural History, London:

At the seven hundred and forty-second meeting of the Biological Society of Washington on January 11, 1930, the following resolution was passed expressing the society's appreciation of the long-continued bibliographical work of C. Davies Sherborn.

WHEREAS, Charles Davies Sherborn, through his bibliographical researches, has made contributions of such outstanding importance in the field of zoology; and,

WHEREAS, He has by continued and painstaking endeavor placed in the hands of his fellow-workers in the "Index Animalium" an instrument of great usefulness; and,

WHEREAS, He has made other notable contributions such as a "Bibliography of the Foraminifera" and an "Index to the Genera and Species of the Foraminifera" and a "Catalogue of British Fossil Vertebrata," and other similar works; therefore, be it

Resolved, That the Biological Society of Washington extends to Charles Davies Sherborn its appreciation and recognition of the service he has rendered to his fellow-workers in science.

The members of the Biological Society of Washington desire you to convey to Mr. Sherborn their most cordial greetings for his health and prosperity and wish to assure him of their continued interest in and appreciation of the value to science of the bibliographical work on which he is engaged.

A. WETMORE,
President
W. H. WHITE,
Corresponding Secretary

NATIONAL RESEARCH FELLOWSHIPS IN THE BIOLOGICAL SCIENCES

THE Board of National Research Fellowships in the Biological Sciences, which includes within its scope the fields of anthropology, psychology, botany, zoology, agriculture and forestry, held its first meeting in 1930 on February 7 and 8, and made ten reappointments and twenty-four new appointments for the academic year 1930–31, as follows:

REAPPOINTMENTS

For Domestic Study

Frederick Bernheim—zoology
Carleton S. Soon—anthropology
Eileen W. Erlanson—botany
G. LaVerne Freeman—psychology
Anna H. Gayton—anthropology
D. A. Johansen—botany
T. J. B. Stier—zoology

For Study Abroad

Donald Keith Adams—psychology
Paul R. Gast—forestry
Dietrich C. Smith—zoology

NEW APPOINTMENTS

For Domestic Study

O. D. Anderson—psychology
George S. Avery, Jr.—botany
G. W. Beadle—agriculture
Harold W. Beams—zoology
Alden S. Crafts—botany
L. W. Gellermann—psychology
F. L. Howard—botany
W. E. Lammerts—botany
R. K. Meyer—zoology
Dorothy K. Postle—psychology
Daniel Raffel—zoology
Hugh M. Raup—botany
T. C. Schneirla—psychology
Eleanor H. Slifer—zoology
C. V. Smythe—biochemistry
Olive G. Stull—zoology
J. H. Tiffin—psychology
T. Elliott Weier—botany
Gene Weltfish—anthropology
John S. Yerakis—psychology

For Study Abroad

F. R. Immer—agriculture
W. M. Krogman—anthropology

C. H. McConnell—zoology
L. T. Steiger—agriculture

The second meeting for further appointments for 1930–31 is planned for about May 1 and 2, and applications for consideration at this meeting should be filed not later than April 1, and earlier if possible. Information and application forms may be obtained from the secretary, Board of National Research Fellowships in the Biological Sciences, National Research Council, Washington, D. C.

FRANK R. LILLIE, *Chairman*

THE INTERNATIONAL ANATOMICAL CONGRESS

THE chairman of the American committee, Professor C. R. Stockard, has received the following general plan of arrangements for the International Anatomical Congress to be held in Amsterdam next summer.

The opening ceremonies of the congress will take place on Monday, August 4. From Tuesday to Friday, the mornings will be devoted to sessions for the presentation of the scientific program, and the afternoons to demonstrations.

During the congress there will be an official reception by the municipality of Amsterdam, and another general reception. The usual dinner of the congress will also be arranged. An excursion to several nearby places of interest is planned for members on Saturday, August 9.

The secretaries of the five associated national anatomical societies will receive the titles of communications and demonstrations from their respective members, and from these several lists of communications the definite program is to be arranged.

There will be exhibits of scientific apparatus, microscopes and materials by the manufacturers from various countries.

Further particulars, with lists of hotels, etc., will be sent out as soon as arrangements are completed, and those expecting to attend the congress may arrange to make hotel reservations through a local committee.

SCIENTIFIC NOTES AND NEWS

THE Henri Poincaré Medal of the Paris Academy of Sciences has been awarded to Louis de Broglie for his work on wave mechanics.

ON the occasion of the annual observance of George Washington's birthday by the University of Pennsylvania, the honorary degree of doctor of laws was conferred on Dr. Theobald Smith, who recently retired as director of the department of animal pathology of the Rockefeller Institute at Princeton University.

"THE NEW YORK FARMERS," an organization of those interested in the advancement of agriculture, has established a medal to be awarded from time to time for outstanding achievement in agriculture. The first award of this medal was made in New York City on January 21 to Dr. L. O. Howard, for more than fifty years connected with the Bureau of Entomology of the Department of Agriculture.

DR. CURTIS F. MARBUT, chief of the soil survey of the Bureau of Chemistry and Soils of the U. S. Department of Agriculture, has been awarded the Cullum Geographical Medal, conferred by the American Geographical Society in recognition of services of special distinction in the field of exploration and geographic research. The presentation was made in New York City on February 25 at a banquet of the society. Major-General George W. Cullum, president of the American Geographical Society from 1877 until his death, in 1892, established the award which bears his name. The first award was in 1896, to Admiral Robert E. Peary, discoverer of the North Pole. Among others receiving it have been Fridtjof Nansen, Sir John Murray, Robert F. Scott and Sir E. H. Shackleton. Dr. Marbut's studies and classification of the soils of Europe, Africa, South America and North America have furnished an important contribution to the soil geography of the world.

THE gold medal of the Chicago Geographical Society has been awarded to Rear Admiral Richard E. Byrd, for his flight over the South Pole and his contributions to the geographical study of the Antarctic. The Helen Culver gold medal was awarded to him by the society following his flight over the North Pole in 1926. Captain Roald Amundsen, discoverer of the South Pole, was the only other recipient of both medals.

PROFESSOR F. PAUL ANDERSON, dean of the college of engineering at the University of Kentucky, was awarded the first gold medal of the American Society of Heating and Ventilating Engineers at its recent Philadelphia meeting. The medal will be given annually to the member of the society "whose work or services performed in the field of heating and ventilating or air conditioning are outstanding."

PROFESSOR E. A. ALLCUTT, of the department of mechanical engineering, University of Toronto, has been awarded a Herbert Akroyd Stuart Prize for his paper entitled, "Further Tests on a Two-stroke Cycle

Oil-engine," which was published in the *Proceedings* of The Institution of Mechanical Engineers, London. The prize was offered for the best paper published in the *Proceedings* within the past three years on the general subject of "The Origin and Development of Heavy Oil-engines."

DR. MARSTON TAYLOR BOGERT, professor of organic chemistry at Columbia University, was elected a foreign member of the Royal Society of Sciences of Bohemia at its recent January meeting. This organization is the oldest of the Czechoslovakian learned societies, and one of the oldest in central Europe, having been founded in the year 1770 by Czech noblemen.

THE Rivers Memorial Medal for 1929 has been awarded by the council of the Royal Anthropological Institute to Mr. J. H. Hutton, of the Indian Civil Service, for his services to anthropology in the field in Assam.

DR. J. G. DAVIDSON, manager of chemical sales of the Carbide and Carbon Chemicals Corporation, New York City, has been elected chairman of the New York section of the American Chemical Society for 1930. He succeeds Professor R. R. Renshaw, of New York University.

DR. ROBERT A. HARPER, professor of botany at Columbia University, and Dr. Henry Hurd Rusby, dean of the College of Pharmacy, will retire at the close of the academic year.

DR. JOSEPH S. AMES was officially inducted without ceremony into the presidency of the Johns Hopkins University at the Commemoration Day exercises on February 22. Dr. Frank J. Goodnow, president emeritus, received the honorary degree of doctor of laws.

DR. HARRY WOODBURN CHASE, president of the University of North Carolina, has been elected president of the University of Illinois to succeed Dr. David Kinley, who retires at the close of the academic year. Dr. Kinley has been president of the university for the last ten years and is now sixty-seven years old. Before going to the University of North Carolina Dr. Chase was professor of psychology at Clark University.

DR. FRANCIS P. GAINES, president of Wake Forest College, North Carolina, has been elected president of Washington and Lee University to succeed Dr. Henry Louis Smith, who has become president emeritus.

CLAUDE BURTON HUTCHISON, professor of agriculture and director of the Giannini Foundation of Agricultural Economics of the University of California, has been appointed dean of the College of Agriculture and director of the Agricultural Experiment Station. Howard Ross Tolley, assistant chief and director of research in the Bureau of Agricultural Economics of the U. S. Department of Agriculture, has been appointed assistant director of the Giannini Foundation.

C. A. DONNEL SENIOR, meteorologist at the Weather Bureau, Chicago, has been appointed to succeed the late Professor Henry J. Cox.

PAUL D. KELLETER, at present administrative assistant to the Federal Farm Board, is announced as the new conservation director of the Wisconsin conservation commission.

DR. WARREN M. SPERRY, assistant professor of biochemistry in the School of Medicine and Dentistry at the University of Rochester, has been appointed assistant professor of chemistry, assigned to pediatrics, at the College of Physicians and Surgeons of Columbia University, and director of chemistry to the Babies' Hospital.

DR. ROBERT ANDREWS MILLIKAN, of the California Institute of Technology, addressed the faculty and student body of Carleton College, on January 9, under the auspices of the Sigma Xi Club. His subject, "The Alleged Sins of Science," was the same as at the recent meeting of the American Association for the Advancement of Science held at Des Moines, Iowa.

PROFESSOR K. T. COMPTON, of Princeton University, lectured before the Physics Club of Pittsburgh on February 12 and 13, on "The Heat Balance at Arc Cathodes," and on "Electron Emission in Accelerating Fields."

PROFESSOR FRANK R. LILLIE, chairman of the department of zoology at the University of Chicago, recently gave an illustrated lecture at the Scripps Institution of Oceanography of the University of California on "The Biology of Sex."

DR. C. J. CHAMBERLAIN, professor emeritus of botany in the University of Chicago, delivered a lecture on February 1 before the Royal Canadian Institute, Toronto, entitled "Tramping through Southern Mexico."

AT the invitation of Sigma Xi of Iowa State College, Professor C. W. Stewart, of the State University of Iowa, spoke in Ames on February 17 and 18 on "The Liquid State of Matter" and on "Experiments in X-ray Diffraction."

DR. HARLAN T. STETSON, director of Perkins Observatory, Ohio Wesleyan University, addressed the Franklin Institute in Philadelphia on February 13 on "The Influence of Sun-spots on Radio Reception." On February 19 he lectured at the American Museum

of Natural History in New York on "Eclipse Hunting." The lecture was illustrated by slides and a film showing the actual progress of the eclipse of the sun of May 9, 1929, as photographed by his expedition in Malaya. The lecture was under the auspices of the Amateur Astronomical Association.

THE Cutter lecture on preventive medicine of Harvard Medical School was given on February 28 by Dr. Lafayette B. Mendel, Sterling professor of physiological chemistry at Yale University, on "Nutrition and Growth." These lectures are given annually under the terms of a bequest from John Clarence Cutter, whose will provided that the lectures so given should be styled the Cutter lectures on preventive medicine, and that they should be delivered in Boston and be free to the medical profession and the press.

THE fourth lecturer at the Johns Hopkins University on the Dohme Foundation was Dr. C. M. A. Stine, chemical director of the E. I. du Pont de Nemours and Company, who spoke before the chemistry staff and graduate students on February 14. He spoke on chemical research now in progress at various universities recently visited in England and Holland, with particular reference to the work of Professor Bone, chief professor of chemical technology at the Imperial College of Science and Technology in London. Following the lecture the customary departmental dinner was attended by about forty students and faculty members. Dr. A. R. L. Dohme spoke on his recent trip to Mexico. The earlier lecturers on this foundation have been Dr. C. E. K. Mees, director of research of Eastman Kodak Company; Mr. W. B. Brockway, controller of the Brown Company, Portland, Maine, and Dr. N. A. Shepard, director of chemical research of the Firestone Tire and Rubber Company.

CORNELL UNIVERSITY will inaugurate a six weeks' summer camp in geology open to ten students beginning on June 16, as a result of gifts made by relatives and friends of the late Professor Henry Shaler Williams, formerly head of the department of geology. The Williams Memorial Fund has enabled the Cornell geologists to establish a camp in central Pennsylvania, near Tyrone. The particular region is regarded as one of the best geologic sections in the Appalachian Mountains, affording unusual opportunities for the study of changes in fossil forms and sedimentation. Dr. C. M. Nevin, of the department, will be in charge of the camp.

THE U. S. Civil Service Commission states that eligible candidates are desired for the position of chief of food control, Food, Drug and Insecticide Administration, Department of Agriculture, Washington, D. C., and to insure the appointment of a thoroughly qualified man the method of competition will be as follows: The qualifications of candidates will be passed upon by a special board of examiners, composed of P. B. Dunbar, assistant chief of the Food, Drug and Insecticide Administration, Department of Agriculture; Dr. Harrison Howe, editor of *Industrial and Engineering Chemistry,* and F. W. Brown, chief of the examining division of the U. S. Civil Service Commission, who will act as chairman of the committee. For the purposes of this examination all these men will be examiners of the Civil Service Commission. The entrance salary is $5,600 a year. Higher-salaried positions are filled through promotion. Applications will be accepted by the Civil Service Commission until March 26.

DR. KARL F. MEYER, director of the University of California Hooper Foundation for Medical Research, has returned from a visit to the east, where he was called for conference with the meat packers of Chicago and other cities over botulism or food poisoning in canned meats.

NEIL M. JUDD, curator of American archeology in the U. S. National Museum, returned to Washington on February 13 from an aerial survey of the prehistoric canals in the Gila and Salt River Valleys, Arizona. Mr. Judd was sent as a representative of the Smithsonian Institution to cooperate with representatives of the Army Air Corps assigned to this survey. Lieutenant Edward Bobzien, pilot, and Sergeant R. A. Stockwell, photographer, of Crissy Field, San Francisco, flying a regulation Douglas O-2-H open-cockpit observation plane equipped with a Fairchild aerial camera, devoted ten days to the photographic work, using Phoenix, Arizona, as a base. Cloudy weather handicapped the aviators during the first week; ground haze and smoke restricted flying time to approximately two hours at midday. Mr. Judd estimates that approximately 400 miles of prehistoric main line canals and laterals were formerly utilized in central Arizona, chiefly in the Gila and Salt River Valleys. Most of these ancient canals have recently been destroyed owing to the extension of agriculture. The purpose of the survey was to prepare a mosaic map of the two valleys in order that permanent record might be made of their prehistoric irrigation systems.

DR. A. J. GROUT, moss specialist, has been appointed to the all-year staff of the Biological Laboratory of the Long Island Biological Association, at Cold Spring Harbor. Dr. Grout has desired to retire from teaching in order to devote his full time to the preparation and publication of a series of monographs, the first of which has already been published, on the mosses of North America. An editorial board has been chosen for "The Moss Flora of North Amer-

ica" composed of Dr. A. J. Grout, Dr. Henry S. Conard, in charge of field botany and plant ecology at the Biological Laboratory and head of the department of botany at Grinnell College; Dr. G. E. Nichols, professor of botany and director of the Marsh Botanical Garden, Yale University, and Dr. O. E. Jennings, head of the department of botany, University of Pittsburgh. This summer Dr. Grout will represent the Biological Laboratory at the International Botanical Congress to be held at Cambridge, England. While abroad he will check up type specimens of American mosses located in European museums.

LEON M. ESTABROOK, of the foreign service of the U. S. Department of Agriculture, who for the last five years has been in charge of the World Census of Agriculture, with headquarters at Rome, has returned to Washington for a temporary assignment in the office of the Secretary of Agriculture. He will assist Dr. A. F. Woods, director of scientific work, in making arrangements with the Department of State and the Pan-American Union for the Inter-American Conference on Agriculture, Forestry and Animal Industry to be held at Washington from September 8 to 20. The conference is the outgrowth of recommendations of the Sixth International Conference of American States, held at Havana in 1928. Its purpose is to consider plant and animal production, and to develop plans for all phases of agricultural cooperation. Each nation in North, Central and South America will be asked to send an official delegate, as well as other delegates who are specialists in agricultural production and marketing. The conference will come one week ahead of the Sixth International Road Congress and it is expected that many of the delegates will attend both conferences. During the two years 1923–24 Mr. Estabrook was loaned by the Department of Agriculture to the Argentine Government to reorganize its service of agricultural economics and statistics. In the last five years in his work on the World Census of Agriculture, Mr. Estabrook visited the capitals and agricultural production centers of every country of the world with the exception of three.

UNDER the will of Mr. George de Arroyave Lopes, the executors have to hand over the residuary estate, estimated as being over £70,000, bequeathed to the Zoological Society of London, to be held by the society as the De Arroyave Fund. The income is to be applied for the upkeep and improvement of the Zoological Gardens and for the objects of the society. The society has had hitherto to depend almost entirely on the subscriptions of its fellows and on the fluctuating income from the gardens.

FIRMIN DESLOGE has made a gift of $1,000,000 to St. Louis University for the erection of a hospital.

IN preparation for the celebration of the one hundredth anniversary of the founding of Lafayette College in 1932 the board of trustees has announced a campaign for $3,500,000 for endowment and buildings. One million dollars has already been subscribed by three members of the board. John Markle, of New York, has given $400,000, in addition to the $500,000 for the John Markle Mining Engineering Hall, which was recently dedicated. Fred Morgan Kirby, of Wilkes-Barre, has given $500,000 for the Kirby Hall of Civil Rights, which will be completed in May, and Thomas Fisher, of Philadelphia, chairman of the campaign committee, has given $100,000 toward the building of dormitories and for endowment purposes.

A GIFT of the professional library of the late Dr. C. F. S. Tate to the School of Medicine of the University of Southern California and the recent acquisition of the large book collection of Dr. Charles W. Bryson have made possible the establishment of a separate medical library by the university medical school. According to an announcement by Dean William D. Cutter, the library will be housed for the present in two rooms in the basement of Bridge Hall, which are now being outfitted. The appointment of Miss Marguerite Campbell, formerly librarian of the Peking Union Medical School, Peking, China, and of the Boston Medical Library, as custodian was also announced. The library will be opened for use in a few weeks, with between four and five thousand volumes available for reference.

DISCUSSION

THE FUTURE OF TAXONOMY

I HAVE just received a statement from the secretary of the Zoological Society of London, calling attention to the inadequacy of the support given to the *Zoological Record* and hinting that unless conditions improve it may be necessary for the Zoological Society to abandon the enterprise. I use the *Record* almost daily, and find it so essential for my work that I am greatly alarmed at the prospect of its discontinuance. In my own case this would not be so serious as in the case of a younger worker, with perhaps half a century of work ahead. To him it would mean, first of all, a great increase in the time consumed in bibliographical work; and secondly, reduced accuracy in his work, as he would certainly miss important publications.

What are the reasons for the present state of affairs? To some extent, I think, the competition of *Biological Abstracts* is responsible. The *Abstracts* covers a large field not dealt with by the *Zoological Record* and in this field is simply invaluable. But its zoological taxonomy appears to me to be not only very incomplete, but also from its manner of publication of comparatively little use. For my own work I find it practically useless. I understand that it was seriously considered that this part of the *Abstracts* might be dropped, and it seems to me that this should be done. As long as it is there, librarians and officials, and even heads of departments, will imagine that it covers the field adequately, and that the *Zoological Record* is unnecessary.

There is, however, a deeper and more important reason for the non-support of the *Record*. It is the lack of interest in taxonomy. For this, I believe, our graduate schools are largely responsible, and perhaps it is not too much to say that in certain respects the graduate school is an enemy of sound science. This is due to the system, not to any particular fault on the part of those who administer it. Consider what we have. A great and increasing number of candidates for the M.A. and Ph.D., together with other less popular degrees. They overrun the departments in the large institutions, and the problem for the professor is to find subjects which these people may study, and on which they can write an acceptable thesis, in one to three years. The actual time available is much less than this statement might suggest because these students have other things to do, and very commonly are employed in the teaching, often handling all the quiz sections and correcting all the examination papers. In this situation, what subjects of research may be profitably chosen? Those which (1) require little previous knowledge, and (2) involve no great breadth of view. It must not be necessary to accumulate a special collection or library, and it is very important that the student should not bother the professor too much. As a typical example, I think of a brilliant girl I know, who was set to cutting off the tails of salamanders, in order to find out whether (under laboratory conditions, certainly not in the wild!) they grew any faster without a tail to support.

I do not mean to say that most of these theses do not possess some value, at least for those doing the work, and it is true that occasionally an important taxonomic monograph, involving many years of study, is accepted for the Ph.D. But broadly speaking neither the spirit nor the methods are those of profound scientific research; and taxonomy, which requires many years (if only that the worker may discover his own mistakes), is out of the question.

What have we left to rely on? We should expect and demand that the scientific departments of the government prepare monographic works on various groups, especially those of economic importance. Some very good work of this type has appeared, but I think not nearly enough. To be concrete, I do not see any valid reason why the Bureau of Entomology, with its really enormous appropriation and abundance of technicians of all sorts, has never given us a monograph of the Coccidae (scale insects and mealy-bugs).

Yet all that the governments can do is not enough, and it would be deplorable if the progress of science depended wholly on governmental agencies. There remains the amateur, the man of the type and spirit of Darwin and Wallace, who loves science and finds in it the means of satisfying the cravings of his mind, intellectual and emotional. It is the amateur who can rejoice in his slowly increasing collection, in the increments of his knowledge. Though he may spend only a few hours a week at his hobby, he becomes a learned man with the passage of time. When there are enough amateurs in a district, they form a society, a fellowship of the disciples.

There is no simple way to attain all these good things. But the first step is to desire them, and if we do that long enough and earnestly enough they will be realized in abundance.

T. D. A. COCKERELL

UNIVERSITY OF COLORADO,
DECEMBER 20, 1929

RECENT CRITICISMS CONCERNING MEIOSIS IN DROSOPHILA MELANOGASTER

FOR the past few years some rather strongly adverse criticisms have been made by E. C. Jeffrey, in which he claims that the meiotic divisions of *Drosophila melanogaster* are atypical, resembling those of certain species hybrids in plants, and that this fly is therefore also a species hybrid.

Nobody who has worked with *Drosophila melanogaster* has taken Jeffrey seriously in respect to his statements. Being eminent in paleobotany, his venture into a specialized field of animal cytology seems to be a long and daring step. Any one familiar with the elementary laws of genetics can readily perceive the inaccuracy of his assertions. I do not intend to answer him on his latest contribution in SCIENCE of December 13, 1929, but I do feel that the cytologic status of *Drosophila melanogaster* should be briefly submitted to those readers of SCIENCE who are not specially versed in cytology or genetics. Having followed Jeffrey's periodic attacks since 1925, I have no desire to enter into any controversy with him, and

my interest in this matter will end with this brief statement. A detailed account of the spermatogenesis of *Drosophila melanogaster* is now ready for the press and will be published in the *Archiv für Zellforschung und mikroskopische Anatomie.*

It may be well advised to discuss first the merits of methods employed. Professor Jeffrey claims that he obtains better preparations with Carnoy's mixture than with Flemming's. It is true that one may obtain fairly good material with Carnoy's, if the duration of fixation is definitely ascertained within the limit of a few minutes. But it is a very tricky fixing agent and for this reason is not as reliable as Flemming's or many other mixtures which are in general use. Flemming's solution is superior to any other fixing agent in its constancy of delicate preservation, and this point of view is held by most cytologists who have worked with *Drosophila melanogaster* (Belar, Bridges, Frolowa, Metz, Stern, etc.). Carnoy's may be an excellent fixative in plant cytology, but it certainly has a limited use in animal cytology. It must also be pointed out that a fixing solution which is good for one species is not necessarily the same for others, and Jeffrey's criticism that Belar used Carnoy's for Nematodes and avoided it for Drosophila demonstrates his inexperience in the methods of cytologic technique. I also must take exception to the assertion that Flemming's fluid does not preserve mitochondria. It was this very fixative which gave rise to the investigation of mitochondria, and our modern work in this phase of cytology is accomplished chiefly by a modification of Flemming's fluid from which the larger part of acetic acid has been removed.

Several years ago (1924) I described the maturation phenomena in the egg of *Drosophila melanogaster.* The first meiotic spindle is present when the spermatozoa enter. Within twenty minutes the polar bodies are formed, and the maturation divisions are so plainly visible in this large egg that I have used such preparations for classroom demonstrations. If Jeffrey's assertions were true and *Drosophila melanogaster* were a species hybrid, the atypical condition in meiosis should show itself most definitely in the meiosis of the egg and not be confined to the spermatogenesis alone.

To consider *melanogaster* a hybrid would necessitate the same view for other Drosophilae, certainly *obscura, virilis, Willistoni* and *funebris,* since their cytology is of the same type as that of *melanogaster,* and also that they give rise to many mutants. Many geneticists have tried to cross closely related Drosophila species with *melanogaster* without success (Morgan, Metz, Sturtevant, etc.). I spent the greater part of one year in such experimentation, going so far as to inseminate the unfertilized egg by means of a micropipette, without being able to secure normal development. All such eggs degenerated in a short time. The interspecific sterility in all Drosophilae is so pronounced that the possibility of ever obtaining viable species-hybrids has been abandoned by most geneticists. If Jeffrey's comparison of *Drosophila melanogaster* to species-hybrids of certain plants were true, where are the two possible species from which *melanogaster* could have been derived? Why is it impossible to cross *melanogaster* with *Willistoni* or *caribbea,* two species which are morphologically and cytologically so much like *melanogaster* that only an expert can tell them apart?

In 1926 Metz published an account of the spermatogenesis of *Drosophila virilis,* including his observations on *melanogaster* and several other species. Though I am inclined to disagree with him in minor details, in the main his figures are correct and are in line with other observers. He showed that meiosis takes place in a normal, typical manner. There is also no doubt about the correctness of Belar's interpretation of the primary spermatocyte. However, it is absurd for Jeffrey to take Belar's figures 5a, 5b, and 5c on plate II of his "Die cytologischen Grundlagen der Vererbung" for the purpose of substantiating his assumptions. These figures represent three aspects of one cell in the first meiotic division, and Jeffrey makes a strong point of the fact that one chromosome is situated on one side, behind the others. Having examined hundreds of such divisions, I have observed this chromosome many times. In 1909 it was identified by Miss Stevens as the XY combination. I have traced it from diakinesis through the first meiosis to the secondary spermatocytes and found it to lag during the entire process. Lagging sex chromosomes are so common in spermatogenesis that such behavior is the rule rather than the exception. It is due to this lagging that Bridges explained nondisjunction which occasionally occurs in *Drosophila melanogaster.*

In addition to the XY complex, the primary spermatocyte contains three other bivalents. Two of these can be readily recognized, but the third consists of chromosomes so small as to resemble a small dot. To make an issue over a chromosomal arrangement in an "equatorial plane" with lagging sex chromosomes and only two fairly large bivalents is to overreach oneself in unimportant detail. But in spite of Jeffrey's assertions to the contrary, the bivalents do come to the approximate center of the first spermatocyte spindle and separate there into univalents. Belar's photograph 5a shows this condition splendidly. The division of the secondary spermatocyte takes place after an interkinesis. The univalents are there separated into four chromatids, so

definite in outline that their identity and number can be ascertained without question.

Jeffrey's divergent results from those of the large number of cytologists who have worked on *Drosophila melanogaster* must be ascribed to his inexperience in Drosophila technique. Judging from his figures, published in previous papers, he apparently mistakes certain cell inclusions which stain black with iron hematoxylin for chromosomes. This difficulty can be overcome when the Feulgen nuclear reaction, preceded by formol-alcohol-acetic acid fixation, is applied.

I wish to repeat that this account is not written in a controversial spirit, but is merely given as a point of information. I also fully realize that the geneticists could disprove Jeffrey's assumptions even better and more effectively than a cytologist, but I doubt whether any one of them would take the time to do so.

Since this article has been written, Guyénot and Naville have published a most thorough account of the spermatogenesis in *Drosophila melanogaster* in *La Cellule,* Vol. xxxix, No. 1, 1929. They also repeated my investigations on maturation divisions of the egg in which they agree entirely with my work. Their criticism of Jeffrey's work is almost a duplicate of mine given above.

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THE "FERTILIZATION" MEMBRANE OF ECHINID OVA

IN SCIENCE[1] for October 11, 1929, Professor A. R. Moore contributes a note on "The Function of the Fertilization Membrane in the Development of the Larva of the Sea-urchin." Against his conclusion I wish to enter a protest.

I had supposed that even beginners in marine embryology realized that the "fertilization"—vitelline—membrane of echinid ova plays no rôle in development after its complete separation from the vitellus. Every such student knows that by centrifuging, pressure, and the like, uninseminated ova of the sea-urchin are easily deformed—can, for example, be pulled out into long strands—with a return to normal form. This is due to the elasticity of the closely adherent vitelline membrane which encloses the almost watery egg contents and which plays a rôle in the metabolism of the egg. After insemination not only does the membrane stand off from the egg; it becomes stiff, brittle and easily removable; it has changed chemically, as Harvey has shown, and it plays no part in the metabolism of the egg. Removal of the membrane (except by micro-dissection?) from the uninseminated egg is practically impossible. Its removal after insemination has been frequently accomplished and this without injury to the egg or impairment of development. Finally, every student of the living sea-urchin egg has doubtless observed its "hatching," *i.e.,* the escape of the swimming form through the ruptured membrane. What justification, then, has Moore for the conclusion in his note concerning the function of the vitelline membrane in development?

Perhaps Moore did not mean the vitelline ("fertilization") membrane. In that case he should have given his note a different title. If, on the other hand, he meant the hyaline plasma layer the statement in his conclusion is superfluous; here again, every student knows that the hyaline plasma layer is part of the developing egg.

In his experiments, Moore finds that after exposure to an isosmotic solution of urea (he does not give the pH of the solution) uninseminated eggs are capable of fertilization and development without the "formation" of either the "fertilization" or hyaline membrane. Obviously, this might mean simply that the preformed cortex which during and after membrane separation builds up the hyaline plasma layer is so injured by urea that the normal cortical changes underlying the separation of the vitelline membrane are abnormal. The result would then be not the failure to "form" hyaline plasma layer but the rapid disintegration of this layer after it "forms."

If Moore's interpretation of his experiment on the effect of urea—namely, that it inhibits formation of the hyaline plasma layer—be correct, then he has been most unfortunate both in the choice of his title and in the statement of his observations.

E. E. JUST

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PEDOLOGY OR SOIL SCIENCE

IN reply to the comments of Dr. William A. Hamor in the January 17, 1930, issue of SCIENCE relative to the use of the term *pedology* to refer to soil science, attention should be called to the fact that pedology was first used by the Russian soil scientists in 1865, over thirty years before the child scientists adopted it. The latter, as Dr. Hamor notes, are using an incorrect spelling of the word. The term they should employ is *paedology* or *paidology.* In view of the prior use of pedology to refer to soil science and as the psychologists are using the word incorrectly and also because of the general acceptance and use of the term in Europe in place of soil science, the American Soil Survey Association at its annual meet-

[1] 70: 360.

ing in Chicago in November formally adopted the term. No other terms suggested are even being considered. Those mentioned by Dr. Hamor are obviously out of the question. It is well known that the child scientists have been using the word, but if they will spell their word correctly there will be no difficulty. Of course it is not expected that any such term will immediately come into general use instead of soil science, which appeals to many of us as quite satisfactory. Perhaps it may never be generally used. But we can be sure that it will be used to some extent from now on in research work, at least.

P. E. BROWN

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QUOTATIONS

MEDICAL EDUCATION IN AMERICA

THE third report of the influential American Commission on Medical Education dealt with general principles. In a supplement to the third report a critical summary is given of some of the changes in procedure which have been put to a practical test in a number of the American schools of medicine, and serve thereby to illustrate certain of the general principles of training emphasized in the earlier report. The schools are referred to by letters of the alphabet, and not by their names, and without considerable knowledge of the local conditions it is therefore very difficult to identify them. The former period of two years' pre-medical work has now been extended to three or even four years in most of the better schools of medicine, which can easily fill their classes with college graduates; and as 95 per cent. of the graduates follow their medical course of four years by a hospital internship of one to two years, it takes about ten years from the date of entering college for the student to become financially independent. The cost of medical education is naturally going up, and the fees paid by students supply, as shown by the average of the figures from sixty-three schools, little more than one third of the total, the difference being met by endowment and from other sources. In the sixty-three schools, 42 per cent. of the budget goes to provide salaries for the whole-time teachers, and 6 per cent. to part-time teachers. During the last few years the total amount of work, as shown by the number of hours required, has been somewhat reduced in the majority of schools; thus in thirteen schools which in 1925 demanded 4,000 or more hours, there has been an average reduction of over 400 hours, but there are wide variations; in twenty schools taken at random the hours required for anatomy varied from 1,267 to 480, for medicine 1,030 to 428, for surgery 660 to 332, and for gynecology and obstetrics 368 to 168. Although much consideration has been given to uniformity and standardization, there is probably far more variation in the curriculums at different schools than is generally assumed. Three types are described. The first is the orthodox standardized curriculum of recent development, intended to familiarize the students, who all take the same courses, with every phase of medical knowledge; the amount of instruction is heavy, and the staff is small. The second type is represented by a small number of schools closely allied with universities, having few students, who have much more freedom, working on graduate rather than on undergraduate lines, and specializing early; the teaching staff is large, and there are full-time clinical instructors. The third group is intermediate between the other two, and offers a comprehensive training for all and special opportunities for a limited few. Teaching in pharmacology is becoming less concerned with pharmacy and more with the physiological action of drugs, thus forming a bridge between physiology and therapeutics. Clinical-pathological conferences, in which the physician in charge of the case first gives the clinical history and diagnosis, and the pathologist then shows the post-mortem conditions, are well established at some schools, as are joint demonstrations of surgical problems by surgeons and anatomists. Efforts are made to remove the reproach that the student does not know how to deal with the sick person; thus at one school 15 per cent. of the fourth-year students follow the daily work of a general practitioner for one or even two months. The teaching of preventive medicine throughout the curriculum is gaining ground, and it is pointed out that pharmacological instruction might well deal with patent medicines and the mischief they do to patients who attempt to treat themselves at a stage when early diagnosis and proper treatment may prevent serious consequences.—*The British Medical Journal.*

SCIENTIFIC APPARATUS AND LABORATORY METHODS

AN INNOVATION IN SCALE CONTROL

INCIDENTAL to a study of the effect of dusting citrus trees with extremely finely divided sulphurs for possible control of the citrus thrips, the discovery has been made that a very high mortality of the citricola scale (*Coccus pseudomagnoliarum* Kuw.) has accom-

panied the use of these sulphurs. The groves where these tests were conducted are located in the San Joaquin Valley—one of the hotter regions of California.

Dusting citrus trees with sulphur has been practiced for a number of years in southern California against the citrus mite, but evidently no records of scale control by this method are to be found. The daily maximum temperatures for southern California are decidedly lower, as a rule, than is the case in central California where our tests were made. At Lindsay, for example, where most of our data were secured, the mean maximum temperature for June is 94.9° and for July is 101.4° Fahrenheit (U. S. Weather Bureau). These high temperatures probably constitute the factor which causes the scale mortality through the accelerated sublimation of the sulphur particles.

The above table briefly summarizes our data bearing on the subject of scale mortality induced by applications of sulphur dust to citrus trees.

In further explanation of the table it should be recorded that all the dustings were applied between April 15 and June 14, inclusive. The period during which the scale hatched was from May 12 to about July 15, so that the sulphur applications were effective against the "crawlers" throughout most of the hatching period.

The gas-purification sulphur consisted of a mixture of 20 per cent. of sulphur recovered in the process of purifying city illuminating gas and 80 per cent. commercial yellow sulphur. In all cases the sulphurs employed were ground to the point where a high percentage of the material would pass through a 200-mesh-per-inch screen, and the majority of the particles through a 300-mesh screen.

The mortality percentages listed in the table do not include natural mortality from climatic or other causes. Each of the groves had an untreated check which provides the basis for the mortality computation.

It is planned to prepare a more complete manuscript to be submitted at some future time dealing with this highly interesting new method of scale control.

E. A. McGREGOR

U. S. BUREAU OF ENTOMOLOGY

TABLE I

MORTALITY OF CITRICOLA SCALE BY DUSTING WITH VERY FINELY DIVIDED SULPHURS

Grove	Type of sulphur	Number of applications	Date of scoring grove	Net scale mortality	
Montague White	Gas-purification	2	June 14*	100.0 per cent.**	
H. A. Campbell	Commercial yellow	2	" 15	90.1 "	
A. T. Margo	"	2	July 7	97.5 "	
"	Gas-purification	2	" 7	91.6 "	
C. C. Chapman	"	2	" 24	96.1 "	
I. A. Baldwin	"	3	" 25	100.0 "	**
Frank Kennedy	"	3	Aug. 8	98.3 "	
Paul Gramm	"	2	Sept. 11	97.2 "	
"	Commercial yellow	2	" 11	96.5 "	
"	"	3	" 13	96.7 "	
Montague White	Gas-purification	3	" 20	96.7 "	
E. I. Brown	"	3	" 23	98.6 "	

* All dates are of 1929.

** Had larger series been employed these values no doubt would have dropped below 100.0 per cent.

Average mortality for 3 dustings ..... 98.1 per cent.
" " " 2 " ..... 95.6 "
" " " all jobs ..... 96.6 "

CULTIVATION OF BACTERIUM TULARENSE

THE literature contains numerous references to the difficulty encountered in the cultivation of *Bacterium tularense*. Francis has shown that this organism grows well on serum dextrose cystin agar. This medium may be prepared by a process simpler than the somewhat complicated method used by Francis. Medium prepared by the process given below produces abundant growths of *Bact. tularense*, even with primary isolations.

Dissolve 0.05 per cent. cystin in water containing 0.2 per cent. Na_2HPO_4, with the aid of heat; add this to dehydrated heart infusion agar to which 1 per cent. dextrose has been added; heat the whole in the water bath to dissolve the agar. If cystin dextrose agar is

desired, tube and sterilize at fifteen pounds steam pressure for fifteen minutes. Should serum cystin dextrose agar be desired, sterilize the medium in a flask at the above pressure for the same time, then cool to 50° C., add 5 per cent. sterile rabbit's blood serum, tube and slant.

FREDERICK W. SHAW
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SPECIAL ARTICLES

THE EFFECTS OF INFRA-RED, VISIBLE AND ULTRA-VIOLET IRRADIATION ON CHANGES IN ELECTRICAL POTENTIALS AND CURRENTS IN PLANTS

MANY theories have been developed to account for the effects of ultra-violet and infra-red radiations on tissues. We cite: (1) the activation of cholesterol in the skin, (2) the ionization of calcium and phosphorus in superficial tissues, (3) the electrical modifications produced in the proteins of the skin and consequent changes in hydrogen-ion concentration and (4) the presence of fluorescing materials in the skin which may act as absorbers of energy of short wave-length with reemission in the form of energy of longer wave-length which, in turn, is utilized by the tissues either as heat or in chemical changes.

The skin is the medium which separates the organism, whether it be an animal or a plant, from its environment. The skin, therefore, is the medium of reception of radiant energy and the seat of transformation or utilization of the incident energy. Since no changes of a physical or chemical character are produced in cells, tissues or organisms unless there is an absorption of energy, and since protoplasm exercises a pronounced absorption of energy of short wave-lengths (such as ultra-violet rays) and of the shorter infra-red radiations which lie close to the region of the visible spectrum, we have felt that an investigation of the electrical changes produced in the leaves of living plants under various types of selective radiation might give some information regarding the physical and chemical changes taking place in plant tissues when exposed to these various kinds of energy.

Potentiometric changes produced by artificial sources of radiant energy.—Sunflowers were used in general for experimental purposes outdoors during the summer; poinsettias were found satisfactory for indoor investigation during the autumn and winter months. Ordinarily electrodes of thin tinfoil were used and attached to the leaves or stems of plants by means of acid-free kaolin after the method of Sen. In investigating potentiometric changes in leaves, both before and after irradiation, one electrode was fastened near the base of the leaf and the other one was placed at or near the tip of the leaf. Leads were then carried to a Leeds and Northrup potentiometer, using a high sensitivity galvanometer. Various spectral filters, such as Corning Glass Company filters of special make and transmitting only infra-red, red, green, blue and ultra-violet rays and so forth, respectively, were employed. Infra-red energy of long and relatively short wave-lengths could be removed from the sources of illumination by water chambers of sufficient depth and possessing quartz bottoms. The visible and infra-red portions could be removed by water cells and solutions of certain substances or by special mediums transmitting selected portions of the ultra-violet light only. The artificial sources of energy used were infra-red generators and arcs rich in ultra-violet irradiation.

The data obtained during the last three years definitely show that marked changes in electromotive forces in leaves are produced by irradiation with infra-red and ultra-violet rays. The potential changes under heat rays from an infra-red generator are usually rapid and values as high as 0.3 volt change in potential have been obtained. The potential changes produced by irradiation with an air-cooled quartz-mercury lamp have been similar to and in the same direction as those produced with infra-red irradiation. The visible portions of the line spectrum of air-cooled quartz-mercury arcs operated at 90 volts did not produce changes, except possibly small changes in the region of the short visible waves (violet end of spectrum). Irradiation with quartz-mercury arcs from which the infra-red energy had been removed by suitable water filters, hence allowing the incidence of visible and ultra-violet rays only, produced potential changes in the same direction as those produced by infra-red rays and of an order ranging from 0.005 to 0.10 volts. These data show that potentiometric changes are produced by infra-red and ultra-violet irradiation.

Potentiometric changes produced by ionized gases near arcs.—Ozone and oxides of nitrogen produced by the quartz-mercury arc or by other metallic arcs, operating at high voltages, through which air was drawn, the ionized or nascent gases then being conducted through suitable channels to the container surrounding the leaf under test, showed marked potentiometric changes and in the same direction as the changes produced by infra-red or ultra-violet irradiation. These observations lend support to the theory which we are advancing that ionization, due directly to ultra-violet light or indirectly to the ionized con-

dition of the gas (air) which forms the medium surrounding the leaf, is largely responsible for the potentiometric changes observed.

Potentiometric changes produced by sunlight.—During the summer of 1929 series of experiments were conducted in which observations, covering periods of eighteen to twenty-four hours, were made simultaneously on the relative humidity, temperature, electromotive force (voltage) and intensity of illumination of sunlight (foot-candles), using sunflowers which were living and growing outdoors. From the data obtained it is apparent that there is a close correlation between changes in temperature and differences in electromotive forces developed in the leaves of sunflowers. Auxiliary experiments, in which relative humidity and temperature could be kept constant while admitting sunlight through ordinary glass windows with southern exposures, were conducted on plants living and growing constantly indoors. These auxiliary experiments showed that there was a definite correlation between the intensity of solar energy incident on the leaf used and the potentiometric differences developed. The conclusion, therefore, apparently is warranted that the changes in electromotive force are dependent on the infra-red portion of sunlight. The absorption of solar infra-red radiations by the atmosphere and the earth's surface produces changes in temperature. Plants also absorb such infra-red radiation. As a result, therefore, it is not the direct influence of temperature as such, but the absorption of energy from a body (the sun), which is an efficient source of radiant energy at a high temperature, that causes changes in electromotive force.

Changes in electrical current.—Voltages from 22.5 to 45 volts were applied at the electrodes attached to a leaf and the currents read with a micro-ammeter. In general, the values of the currents were of the order of 25 to 50 millionths of an ampere, indicating that the resistances of the leaves were approximately a million ohms each. The currents, under such condition, fall away asymptotically with time. When the approximately steady state of electrical current was reached, leaves were irradiated successively with various portions of ultra-violet, visible and infra-red radiations. The experimental data show that both ultra-violet and infra-red rays (from sources of fairly high temperature radiation) produce increases of current or corresponding decreases of resistance, whereas various selective regions of visible light produce little if any change. There is evidence, therefore, that stimulation by ultra-violet and infra-red rays produces an increase in ions in the leaf and that there may be a change in permeability of membranes.

Spectrophotometric changes.—The spectrophotometer built by Keuffel and Esser was used in the study of the changes in color produced in leaves when exposed to radiation of various qualities and quantities. The type of water-cooled attachment, devised by Brunsting and Sheard, carrying the magnesium carbonate block which serves as a standard for reflection of light, was employed in this work because it enabled us to obtain spectrophotometric measurements from living leaves, intact with the plant, over as many periods and under as many conditions as desired.

After a few minutes' exposure of a given area (usually 2 by 4 cm) on a leaf (intact plant) to infra-red irradiation it was found that considerable darkening of the exposed area occurred shortly after irradiation. The degree of this darkening increased with time until it finally reached a steady state. Repeated exposures of various leaves of different plants to irradiation with red, yellow, green and blue portions of the visible spectrum did not produce changes in spectrophotometric reflection. Ultra-violet rays, especially those below the limit of sunlight (practically speaking, 300 millimicrons), produced a definite bronze or brown-black effect in general. Hence plants, as well as human beings, get a browned and darkened epidermis by exposure to ultra-violet rays. Microscopic examinations showed that these effects were accompanied with a breaking down of the outer layers of cells of the affected areas.

CONCLUSIONS

(1) Marked changes in the differences of potential between the base and tip of leaves intact with plants can be produced by ultra-violet and infra-red radiations.

(2) No change in differences of potential in intact leaves can be observed under irradiation by selected portions of visible light or by the total visible energy of sunlight, with the possible exception of energy possessing wave-lengths lying at the extreme violet and red ends of the spectrum.

(3) In general, the difference of potential between the base and tip of the leaf, when the plant is kept continuously in darkness or subdued daylight, is such that the base of the leaf is relatively positive with respect to the tip of the leaf. The greatest difference of potential (base positive and tip negative) arises when the plant is kept in darkness.

(4) Under high artificial illumination or under noon-day sunlight, when the plants are housed behind ordinary window glass, the difference of potential decreases and the tip of the leaf may become positive relative to the base of the leaf.

(5) Very marked and rapidly produced differences of potential in leaves may be obtained by exposing the leaf (with electrodes protected) to gases drawn from the vicinity of quartz-mercury or other metallic arcs.

(6) From the data obtained relative to humidity, temperature, foot-candles of illumination and potential differences in leaves of plants (sunflowers) growing outdoors, it is concluded that these electromotive forces between base and tip of leaves are due chiefly to infra-red radiations.

(7) When an external and constant electromotive force is impressed on the leaf across the two electrodes, the values of the currents in the circuit including the leaf are increased by either ultra-violet or infra-red radiation or the values of the resistances are decreased.

(8) No measurable changes in currents under an impressed electromotive force are produced in leaves when exposed to various regions of visible light.

(9) Leaves may be darkened or tanned by ultra-violet or infra-red radiations. The character of the bronzing or tanning effect is apparently somewhat different in the two cases. Both ultra-violet and infra-red rays, however, possess the ability to fix the chlorophyll in the tissues, probably by disruption of cells and localized dehydration of surface tissues.

(10) From these experiments and other investigations which have been conducted on germination of seeds, growth of plants and development of chlorophyll, we conclude that the phenomenon of growth is evidenced, in part at least, by changes in electromotive force, and is largely dependent on and stimulated by the ultra-violet and infra-red regions of sunlight. The visible portions of sunlight are used, in all probability, for the development of chlorophyll.

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THE MANGANESE CONTENT OF THE MISSISSIPPI RIVER WATER AT FAIRPORT, IOWA

THE fact that the normal growth of plants, in many instances, depends on the presence of small amounts of certain metals is fast becoming apparent. Felix (1927)[1] reported that the application of copper improved certain muck soils on which onions and lettuce were raised. Brenchley (1910)[2] found that manganese stimulated the growth of barley, and Deatrick (1919)[1] found that manganese prevented chlorosis in wheat.

It was with these results in mind that the writer undertook to investigate the manganese content of the Mississippi River water at Fairport, Iowa. The Mississippi River furnishes the water for the fish ponds at this place. It was thought that possibly manganese might be a limiting factor in the production of algae in the fish ponds.

In Table I are given the results of some manganese determinations made during the summer of 1929. The results show that while the quantity varies considerably from time to time, there is always an appreciable amount of manganese present. It does, therefore, not seem likely that manganese was a limiting factor.

TABLE I

SHOWING AMOUNT OF MANGANESE IN PARTS PER MILLION. THE LETTER S REFERS TO SURFACE SAMPLES, THE LETTER B TO BOTTOM SAMPLES

Date	Parts per million	Date	Parts per million
5/24/29	S. .044	9/21/29	S. .120
			B. .120
6/12/29	S. .106	9/27/29	S. .080
7/2/29	S. .055	10/5/29	S. .128
			B. .100
7/16/29	S. .075	10/12/29	S. .096
			B. .088
8/7/29	S. .114	10/19/29	S. .080
	B. .084		B. .072

The results shown in Table I were obtained from samples of filtered water, and they presumably represent the manganese that was in solution and not the total manganese.

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[1] Raber, "Principles of Plant Physiology," Macmillan, 1928.
[2] *Ann. Bot.*, 24: 571, 1910.

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